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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct and disequilibrium effects on precipitation in transient climates" *by* D. McInerney and E. Moyer

Anonymous Referee #2

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Summary:

In this paper, the authors use climate model simulations (both existing CMIP3 simulations and new CCSM3 simulations which they carried out) to address the issue of why the transient global precipitation (P) response to temperature (T) changes is less than the equilibrium response. The simulations analyzed were driven by a variety of different CO2 and solar forcings. It is concluded that ocean heat uptake is the dominant driver of transient variability in the P-T relationship, rather than the direct effects of radiative forcing agents as has been suggested in several previous studies.

This is to some extent a novel study that has some strong points. In particular, I applaud the authors for the idea of using regional information to try to better understand global mean changes. In the end, though, I do not believe that their conclusion of a dominant

role for ocean heat uptake follows from their analysis. On the contrary, their results to me seem to be more supportive of the idea that the direct effects of radiative forcing are the primary factor producing transient variability in the P-T relationship. As such, I do not believe that the paper in its present form should be published in ACP.

In my three major comments below, I will discuss in greater detail some of the issues that I had with the authors' analysis and their conclusions that followed from it. First, though, I would like to say that I believe that the manuscript could be improved if more discussion was devoted to explaining why physically we should expect ocean heat uptake to produce transient variability in the P-T relationship. I do not agree with the authors' explanation (p. 19652, lines 8-16) that ocean heat uptake "could be interpreted as relating to transient cooling at the ocean surface", which "reduces the energy available for evaporation and hence precipitation". How can ocean heat uptake (i.e., a positive energy imbalance at the surface) be accompanied by surface cooling? I agree that it would act to suppress the rate of surface warming over the ocean relative to land areas, since the heat added at the ocean surface can be mixed over a much greater depth – but the ocean surface would not actually cool. It is this effect of ocean heat uptake on the regional pattern of surface T change, in fact, that is invoked by Allen and Ingram (2002) to explain transient variability in the P-T relationship. Specifically, in a transient climate, the regional pattern of surface T change associated with a given change in global mean surface T is different than the regional pattern of surface T change at equilibrium. This produces different sensitivities in the two cases of the atmospheric radiative cooling (and thus P) to changes in global mean T. We can consider the equation for global precipitation changes ΔP that is given by Allen and Ingram (2002) (reproduced in the present study in equation (1)). The ocean heat uptake term in this equation (cN), as far as I can tell, is essentially a 'correction' term for disequilibrium. It is needed because the coefficient 'a' in Allen and Ingram (2002) (which represents the sensitivity of the atmospheric radiative cooling to global T changes) is assumed to take on its equilibrium value. However, if the transient value(s) of the coefficient 'a' was to be used instead, would we still need the ocean heat uptake term in equation (1) to correctly predict ΔP?

In any event, I hope that the authors will consider these comments, and those given below.

Major comments:

1) In Section 3, the authors examine the P-T relationship separately for the global ocean and land domains using output from different climate model experiments. They conclude that since this relationship exhibits greater transient variability over the ocean, the ocean heat uptake must be the primary driver of the variability, as opposed to the direct effects of radiative forcing agents. I don't agree with this assertion, and, in fact, the authors' results suggest to me that the direct effects of radiative forcing are the dominant driver of the transient variability in the P-T relationship (or at the very least, the results are consistent with this explanation). This is because the P-T relationship changes abruptly (particularly over the ocean) once atmospheric CO_2 concentrations are stabilized (Figures 2 and 3), which is consistent with a fast response of the atmospheric radiative cooling (and thus P) to the stabilization of the forcing. The ocean heat uptake, however, will continue for decades to centuries (or longer) after the forcing is stabilized, due to the inertia of the ocean. If the ocean heat uptake is in fact driving transient variability in the P-T relationship, as the authors claim, then there should be some indication of this variability in the period after the forcing is stabilized (when ocean heat uptake is continuing). I see no evidence for this in Figures 2-4, though, with P versus T appearing to have a constant slope following forcing stabilization.

2) In Section 4, the authors discuss equations for the global precipitation change ΔP that were derived in Section 1 (equations 2-4). Equation (2) states that ΔP arises due to T changes ($a\Delta T$), the direct effects of radiative forcing ($b\Delta Teq$), and ocean heat uptake ($c(\Delta Teq-\Delta T)$). (The equation is based on an analogous expression presented by Allen and Ingram (2002).) The authors then go on to rewrite equation (2) in two alternative forms. In one form (equation 3), they claim that "transient precipitation is

driven only by climate disequilibrium" (assumed to be proportional to ocean heat uptake). In the other form (equation 4), they claim that "the only driver [of transient P] is a direct effect of the forcing agent". In Section 4, the authors evaluate the correlations between the coefficients in equations 3-4 and based on this, they conclude that equation (3) is more physically meaningful since its coefficients are not significantly correlated with each other. I don't quite understand this. Equations (3) and (4) are exactly the same equation, just written two different ways. How then can one be any more physically meaningful than the other? The authors refer to equations 3-4 as "end-member cases", i.e., suggesting that equation (3) includes only ocean heat uptake effects on ΔP (and excludes direct effects of radiative forcing), while equation (4) includes only the direct effects of radiative forcing. This is not correct, however. Both equations (3) and (4) include ocean heat uptake AND direct forcing effects; in equation (3), the direct forcing effects are simply rolled into the coefficients, while in equation (4) the ocean heat uptake effects are rolled into the coefficients. (The physical meaning of these coefficients, by the way, seems rather unclear.) Perhaps I am just missing something here, in which case I welcome clarification by the authors...

3) In Section 6, the authors discuss the evolution of the surface energy budget in CCSM3 simulations driven by instantaneous CO_2 and solar forcings. They calculate the ocean heat uptake as the residual in the energy budget (i.e., the sum of the surface radiative and non-radiative fluxes). I have one major issue with the discussion here. Specifically, the authors seem to imply that the initial reduction of surface latent heat flux and P suppression that occur in the CCSM3 runs are a response to ocean heat uptake (e.g., p. 19667, lines 6-10). This does not physically make sense, however, as ocean heat uptake (and the associated ocean warming) would act to increase the latent heat flux (and thus P) in order to remove the positive energy imbalance at the surface. Clearly, the initial reduction in latent heat flux in the CCSM3 runs must be the cause of the ocean heat uptake rather than a response to it. (Of course, changes in other surface energy fluxes also contribute to the ocean heat uptake, such as, e.g., the increased shortwave radiation input in the solar-forced runs.)

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19649, 2012.